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THE SCOPE OF PROTOZOOLOGY[1]

TWENTY-ONE years ago when I first began the study of protozoa, biologists in general were inclined to look upon these animals mainly as a means of entertaining amateur microscopists in their idle hours. Since then the subject has developed in widely different directions and protozoa have found a place in the deeper problems of biology; indeed, they are considered important enough to warrant the establishment of several chairs of protozoology in different parts of the world.

I am frequently asked to tell what protozoology is, and occasionally find difficulty in correcting the impression that a protozoologist is a primitive and undeveloped zoologist; but difficult as this sometimes is, I find even greater difficulty in giving an adequate idea of the scope of protozoology. I have chosen, therefore, as the subject of this lecture, this very general topic. In it I have no pet hypothesis to develop, nor scientific nut to crack, but desire only to point out the nature of the work done in protozoology as a basis for a definition of its scope.

Up to 1890 the work on protozoa was largely descriptive. The first discoveries by Leeuwenhoek in 1675 gave a new lease of life to the theory of spontaneous generation which had received some hard knocks through the direct experiments of Redi, Malpighi and Harvey. The new discoveries with the microscope merely added fuel to the fire of the later nature philosophers, which, however, mostly went up as smoke theories, such as that of organic transmi-

[1] Lecture delivered at the Marine Biological Laboratory, June 30, 1911.

gration, as developed by Buffon in France, and Needham in England. These naturalists saw in the Leeuwenhoek animalculæ only the disintegrated and free-living parts of higher animals and plants. It can not be stated positively, but there is nevertheless some reason for believing that the smouldering embers of this philosophic fire were kept alive by Oken and Goldfuss in Germany, and by Bichât in France and finally fanned into the full blaze of the cell theory by Schleiden and Schwann, ninety years afterwards.

In the meantime the work of O. F. Müller (1786), and especially that of C. G. Ehrenberg (1833–1838) and F. Dujardin (1835–1841) had resulted in some taxonomic order amongst these microscopic forms which Cuvier had generously included in the animal kingdom under the name of chaos animalculæ. Other important steps were taken by von Siebold in 1845 who first described protozoa as single-celled organisms; by Max Schultze in 1863, who showed that the living substance "sarcode" of protozoa is the same as the living substance "protoplasm" of higher animals; and by Bütschli in 1875 who gave the final evidence in support of the unicellular nature of protozoa by showing that the nucleus of the protozoan cell is similar to that of the tissue or egg cell, and like the latter, divides by karyokinesis.

Bütschli's later work of 1882–88 gave the real ground work on which modern protozoology rests. Summarizing all of the preceding discoveries and bringing together the disconnected observations and theories of his predecessors, he gave us in these approximately 1,700 pages of acute criticism careful observations, lucid descriptions and logical deductions, a masterful zoological treatise such as rarely appears in these days.

I have arbitrarily chosen the year 1890 as a dividing point in the history of protozoology. Before this the work was chiefly descriptive and taxonomic, after this it became more speculative and experimental, although it also developed along the quite unexpected lines of practical biology and public hygiene. For my purpose here I shall not speak of the splendid descriptive work, especially on parasitic forms, that has been done since 1890, but will devote my time to a short statement of the activities in certain other lines of protozoology, especially the cytologic, pathogenic and general biological.

I. THE CYTOLOGIC SIDE

In a strict sense all work on protozoa might be classed as cytological since it has to do with the single cell. But there are two ways of looking at these cells. We may regard them, on the one hand, as morphological units of structure comparable with the single tissue cell, or, on the other hand and following Whitman in his interpretation of the egg cell as an organism, we may regard them as complete organisms performing all of the functions of higher animals. Looked at from this point of view the inadequacy of the cell theory as applied to protozoa is obvious.

In a strictly morphological sense then protozoology includes the study of cell structures homologous with the morphological elements of egg and tissue cells—but these structures are more primitive, more generalized, and, in a sense, more easily correlated with their functions in the cell.

First, as to the structure of protoplasm. We are generally agreed at present that it is inaccurate to speak of any one structure as common to all protoplasm, but many cytologists, amongst whom Bütschli, working chiefly on protozoa, was the first, be-

lieve that the different types are referable to one common generalized type which Bütschli described as alveolar in structure. A simple example of such modification of the alveolar into denser plasm can be easily demonstrated in the protruding pseudopodium of *Amœba proteus.* Here the endoplasmic alveoli become drawn out into ellipsoidal forms, the alveolar walls come together and fuse, forming the characteristic denser ectoplasm. Another good example of the same metamorphosis may be seen in the formation of the temporary membrane which appears between the ectoplasm and the endoplasm of *Actinosphærium eichhornii.*

Second, as to nuclei. The study of protozoan nuclei has taught us that a definite, formed nucleus is not essential for cell life. There are many cases amongst the protozoa where there is no morphological nucleus, but the functions of this organoid of the cell are presumably performed by fragments of chromatin distributed throughout the protoplasm. Such is the case, for example, in *Dileptus gigas,* where each granule at cell division elongates and divides. When formed nuclei are present they are provided with a firm and thick membrane which does not disappear during division as in nuclei of higher animals and plants. The chromatin also, is not arranged in a reticulum as in higher forms, but is usually massed in one or several solid bodies termed karyosomes. These have often been called chromosomes, but such use of the term is incorrect, for these karyosomes in many cases break down into finer granules which are secondarily fused into elements strictly homologous with chromosomes of higher forms. In the protozoa therefore we have abundant material for working out a possible evolution of these important elements of higher cells, from generalized conditions of the parasitic amœbæ to the formation of primitive chromosomes in *Noctiluca* or *Paramecium.* In such primitive forms the number of chromosomes is always greater than in metazoa, more than two hundred having been counted in *Paramecium caudatum.*

Third, as to the centrosome. Cytological study of protozoa gives much more direct evidence of the function of this organ of the cell than does its study in egg or tissue cells. In protozoa it is undoubtedly a kinetic center of the cell in the sense of being the central organ in different types of movements. Many types of Heliozoa, such as *Acanthocystis* or *Sphærastrum,* have a definite central granule in the resting cell. At division periods this divides and forms a spindle; the nucleus is drawn into the nuclear plate and connected by fibers with the divided centrosome, and the outcome is a typical karyokinetic figure. After division the spindle fibers and astral rays grow out from the central granule to form the axial filaments of the actinopodia, which in some species of *Acanthocystis* and *Artodiscus* have a vigorous springing movement. In *Dimorpha* both actinopodia and flagella are present and, both having the same origin, we are led to the conclusion that flagella, in this case at least, are little more than naked axial filaments. Similarly, in various types of flagellates, *e. g., Trypanosoma, Herpetomonas, Crithidia,* etc., the flagellum forms by outgrowth from the centrosome thus proving the intimate connection between the locomotor apparatus of the organism and its centrosome.

In many cases this kinetic center is inside of the nucleus—giving what Boveri called the centronucleus type of nucleus. In such cases the axial filaments of *Heliozoa* abut against the nuclear membrane (*e. g.,* in *Actinophrys, Actinosphærium, Camptonema,* etc.), and during division

the intra-nuclear centrosome divides first. In all cases the kinetic center appears to be formed from chromatin, or at least from nuclear material and seems to be made up of a special kind of nucleoplasm. Frequently, as in *Trypanosoma, Trypanoplasma* and allied forms, the kinetic center emerges from the nucleus as in *Heliozoa,* but is accompanied by a small amount of chromatin thus forming a second nucleus which Woodcock has aptly named the kinetonucleus. Such double nuclei, which, it may be pointed out, are in no way homologous with the dimorphic nuclei of infusoria, have led Hartmann, Nägler, Prowazek and some others to form a special group of protozoa termed the *Binuclearia.* The point of view leading to this artificial group has been ably criticized by Dobell.

Fourth, as to chromidia. Goldschmidt and others of the Munich school have interpreted a number of indeterminate structures of tissue cells as chromidia or granules of chromatin discharged from the nucleus. Waiving the question for the present as to whether such objects are chromidia or chondriosomes of unknown origin, there is no doubt whatever that chromidia of nuclear origin occur in protozoa and play a most important rôle in their vital processes. In rhizopods especially, chromidia are formed during, or prior to, the period of maturity, by nuclear secretion, nuclear dissolution or nuclear fragmentation, the granules becoming individually, or after fusion, the nuclei of conjugating gametes. It thus becomes possible to speak of a special germ plasm in protozoa as distinct from somatic plasm. Such chromidia are to be distinguished from the products of nuclear degeneration which occur under abnormal conditions of feeding or environment and which are more analogous to nuclear degeneration and granulation-tissue formation in higher animals.

There remain many lines of research in protozoan cytology, especially in the direction of maturation and fertilization phenomena, only a few forms having been adequately studied. The enigmatical third division in maturation has evidently some connection with sex, since this division is heteropolar in *Didinium, Paramecium caudatum* and *P. bursaria,* the smaller nucleus migrating, the other stationary, during conjugation. Splendid results lie at the end of patient study in this line of research.

II. THE PATHOGENIC SIDE

The development of this branch of protozoa study was so rapid and so spectacular and seemed to arise so unexpectedly out of a clear field, that many investigators, especially pathologists and other medical men, are inclined to regard it as constituting the whole of protozoology. Up to 1890 only two human diseases were suspected of being caused by protozoa. These were dysentery and malaria. To-day more than fifteen human diseases are known or suspected to be of protozoan origin.

Parasitic amœbæ were first observed in the human intestine in victims of dysentery by Lösch in 1875. He had no hesitation in claiming them to be the cause of dysentery and named the organism *Amœba coli.* Other pathologists, however, soon found similar organisms in the intestines of normal men and Lösch's claim was discredited. Councilman and Lafleur in 1891 found two types of amœbæ, one of which—*A. coli*—was considered a harmless commensal, the other, which they called *Amœba dysenteriæ,* they claimed to be the cause of tropical dysentery. Casagrandi and Barbagallo in 1897 were the first to actually prove that the *coli* form is

harmless. They also suggested the new generic name *Entamœba* for these parasitic amœbæ, believing that the differences between them and free forms like *Amœba proteus* are great enough to justify a generic distinction. In this they were followed by Schaudinn in 1903, who succeeded in causing dysentery in cats by feeding them with isolated cysts of the pernicious type which, ignoring the prior specific name *dysenteriæ,* he called *Entamœba histolytica.* The harmless type he called *Entamœba coli* and confirmed Casagrandi and Barbagallo by repeated experiments on cats and upon himself.

Similarly with malaria a few observations were made prior to 1890, but the most valuable work was done after that date. In 1881 Laveran, a French military doctor in Algiers, discovered organisms in the blood of malaria victims. He announced them as the cause of malaria under the name *Oscillaria malariæ,* this generic name being changed four years later to the more incongruous name of plasmodium by Marchiafava and Celli. Another important point was made by Golgi in 1886, in demonstrating that the characteristic paroxysms of the victim coincide with the simultaneous reproduction of the parasites.

It is impossible here, to give the names of the scores of observers who have added some point or other in connection with these parasitic organisms, or to give credit for the first suggestion as to their mode of transmission. After the facts of transmission were proved, numerous claimants of the honor of first suggesting the possibility of mosquitoes carrying malaria or yellow fever, turned up. Theirs is but an empty honor, however, and I dare say they are entitled to all the glory they can get from proclaiming their clairvoyance from the house tops. We are, however, justified in having no little national pride in the fact that two of our countrymen, Smith and Kilbourne, in 1893 actually proved for the first time the transmission of disease-causing protozoa by blood-sucking arthropods. The honor for their discoveries and patient observations and experiments on Babesia in connection with Texas fever in cattle was not shouted from the ridge pole, but came with the fact that their results were immediately applied to human diseases. To Smith and Kilbourne, then, belong a great part of the credit and honor of paving the way to the present-day control of malaria and sleeping sickness, and the practical extinction of yellow fever in epidemic form.

The repeated suggestions that mosquitoes might transmit malaria were brilliantly proved true by Ross in India in 1897–99, and Grassi, Bignami and Bastianelli in 1898–99 in Italy. The former showed that bird malaria is transmitted only by species of *Culex,* the others, that various types of human malaria are transmitted solely by species of *Anopheles.* Stages in development of the parasites in the mosquitoes were made out by Grassi and others, and the last step was taken in the direction of proof by Schaudinn, who, in 1902, watched under the microscope, the penetration of his own blood corpuscles by sporozoites fresh from the proboscis of an infected mosquito.

The transmission of yellow fever by mosquitoes of the genus *Stegomyia* was proved in 1900–01 by the American commission consisting of Reed, Carroll, Agramonte and Lazear, and so clearly and minutely was the prophylactic routine worked out, that epidemics of yellow fever are now a matter of history. Should one occur in any civilized community, it would surely indicate ignorance or criminal carelessness on the part of the health authori-

ties. The cause of yellow fever, however, is still unknown; when discovered, the cure for the disease will surely follow just as its prevention followed the discovery of its mode of transmission.

After the malaria problems were cleared up, discoveries of other protozoan diseases followed in quick succession. Kala azar, dum dum fever, oriental sore and allied diseases of the far east, were found by Leishman, Donovan, Wright, Christophers, Patton and others, to be due to a flagellated protozoon of the genus *Herpetomonas,* and transmitted by bed bugs.

Sleeping sickness, the great scourge of central Africa, was hunted down by the indefatigable David Bruce in 1903, who showed that it is transmitted by a tse tse fly, *Glossina palpalis.* This discovery followed his brilliant researches of 1894–97 when he traced the cattle disease called "nagana" and the "tse tse fly disease" of cattle to the same protozoon—*Trypanosoma brucei*—and showed that a tse tse fly—*Glossina morsitans*—is the intermediate host. The final observations on human sleeping sickness were possible through the earlier discoveries by Lewis in 1879 on a trypanosome of the rat; by Forde (1901) and Dutton (1902) of a trypanosome in victims of Gambia fever which was regarded up to that time as distinct from sleeping sickness. This organism was named by Dutton *Trypanosoma gambiense.* Also, in 1903, Castellani discovered a trypanosome in the cerebrospinal fluid of victims of sleeping sickness and named it *Trypanosoma ugandense.* Bruce showed that the trypanosomes of the two diseases are the same and that Gambia fever is the initial phase of the fatal disease.

Time does not permit even the naming of other species of trypanosomes found in warm- and cold-blooded animals; nor of the many researches that have resulted in the discovery of intermediate hosts amongst leeches, flies and lice. Much has certainly been accomplished, but there still remains a great and undeveloped field for research in the life histories of the various species.

Perhaps the most spectacular discovery in connection with protozoa and disease was that of Schaudinn in 1905, when in a short publication he announced the discovery of spirochætes in syphilitic lesions. This modest little paper of four or five pages has been the inspiration of thousands of titles, most of which have added little or nothing to Schaudinn's original work, the majority dealing with technical methods, a few with morphological changes and the life history, and a few, notably Robert Koch's, with treatment. Other spirochæte diseases, such as yaws or frambesia, human relapsing fever and tick fever, or diseases of cattle and poultry, have been shown to be transmitted by ticks of one species or other, but *Treponema pallidum,* as Schaudinn finally called the spirochæte in syphilis, is apparently transmitted solely by contact.

One of my students this spring made the comment that most of the references I had given in connection with pathogenic protozoa seemed to fall within the period of 1900–05. The observation was entirely correct and the fact is undeniable that the last five years have given little of value in this branch of protozoology, while in the preceding five-year period not only were the majority of protozoan diseases discovered and their means of transmission established, but that period gave us Mesnil and Mouton's method of cultivating parasitic amœbæ on artificial media, and the brilliant researches of Novy and MacNeal resulting in an entirely new method for the study of parasitic flagellates. Since that period few new discoveries

have been made; culture methods have been extended to the spirochætes and some good observations have been made on the interrelationships of parasitic flagellates and hæmosporidia. In my opinion, however, this branch of protozoology has seen its period of greatest development and, save for the working out of life histories, the protozoologist may well turn over the pathogenic protozoa to the departments of medicine, public hygiene and public sanitation.

In preparing this lecture I was tempted to dwell longer on this interesting and important phase of protozoology and to give a detailed account of the trials and difficulties experienced in establishing the causes of protozoan diseases. Also I should like to speak at length on the probable causes of smallpox, scarlet fever, rabies, trachoma and molluscum contagiosum, and about the many fruitless attempts to trace human cancer to protozoa, but I must hasten on to a third, and, as I believe, the most important, branch of protozoology, general biology.

III. THE BIOLOGICAL SIDE

Here the field of protozoology expands so widely that I can speak of only a few topics, for the problems are fundamental and universal and merge into those which every biologist is striving to solve.

Verworn in 1888 made the statement that protozoa seem to have been especially adapted by nature for the purposes of the physiologist, for here, in the single cell, are performed all of the functions which higher animals perform. This was twenty-three years ago and the fact that strikes us to-day is that, in spite of the vast amount of work done in the subject, these same fundamental vital activities remain almost as obscure as they were then. Some progress, nevertheless, has been made. The early experiments of Balbiani, Verworn, Gruber, Hofer and a score of others demonstrated that enucleate fragments of cells could not secrete, grow nor continue to live, while Verworn in 1891 showed that the isolated nucleus is equally impotent. The axiom was thus laid down that nucleus and cytoplasm are equally important for the proper performance of vital activities.

At this earlier period it was thought that great light would be thrown upon the vital functions of higher animals through study of the simpler activities in protozoa, especially in the directions of (1) digestion and assimilation, (2) irritability, (3) growth and reproduction, (4) regeneration, (5) sex and fertilization, (6) death and physical immortality, etc., but it was soon discovered that under the mask of simplicity lie hidden the same great problems which puzzle biologists in every other field of study. Let me illustrate briefly some of these points.

1. *Digestion and Assimilation.*—The early observations by Le Dantec, Meissner, Fabre-Domergue, Greenwood and others from 1888–1894 demonstrated the presence of some mineral acid in connection with proteid digestion in different types of protozoa, and it was suggested that some simple ferment, acting in an acid medium, is responsible for digestion in these single cells. This suggestion was confirmed by Hartog and Dixon in 1901, who isolated a proteolytic ferment active in an acid medium; but the subject became more complicated when Mouton and Mesnil in 1902–03 isolated a proteolytic ferment that was active in an alkaline medium, and suggested that the digestive ferment in protozoa is more like trypsin than pepsin. Finally, Nierenstein and Metalnikoff, in 1903–07 showed that both types of ferment are involved, digestion beginning with

an acid reaction, followed by an alkaline reaction, and conforming in a general way with the digestive processes in higher animals. Few physiologists have attacked the problem of assimilation in protozoa. Verworn, however, in his "Biogenhypothese," has outlined a theoretical conception of the combination of protoplasm molecules with the products of proteid digestion and based on the Ehrlich side-chain hypothesis.

2. *Irritability.*—Jennings's splendid studies on the behavior of protozoa and lower metazoa have shown that all forms can not be interpreted as simple units of protoplasm reacting to all external stimuli by the same simple reflex. A *Poteriodendron,* on its simple protoplasmic and filamentous stalk, has but the one reaction, contraction of the stalk, but a *Stentor, Vorticella* or *Paramecium* has not only one but several forms of reaction which are frequently so coordinated as to defy analysis. The reactions, furthermore, vary apparently with the physiological state, or, presumably, with physical and chemical states of the protoplasm. Protozoa are thus similar to the lower metazoa and, with them, have been drawn into the field of comparative psychology.

3. *Growth and Reproduction.*—Spencer's theory of growth and reproduction was soon found to be as unenlightening with protozoa as with higher forms and deeper interpretations have been sought. Few have undertaken to formulate any theory of cell division from protozoa alone, but Hertwig in 1902 advanced a physical theory of growth and division based on his protozoa studies, which has had no little influence. This is now known as the "Kernplasmaspannungstheorie," or the nucleus-plasma-tension theory. Briefly stated, this theory is based upon the view that the ratio of nuclear mass to cytoplasmic mass is constant under certain normal conditions of the cell, and may be expressed by the ratio N/P. If either factor is increased without increase of the other, an "abnormal" condition ensues. If the P factor increases, as it does with growth, an increasing tension in the cell results in a disturbance of the nuclear conditions and an incitation to regulation by division. If, on the other hand, the nucleus plasma ratio is changed to the advantage of the nucleus, chromidia formation and cell degeneration are the outcome.

The bare statement of this theory makes it appear crude and infertile, for it is difficult to see how mass relations can be the cause of growth, division or depression, but if we see in the varying ratio of nucleus to cytoplasm only an index of the chemical interchange going on all the time between the several parts of the cell, and interpret such variations as effects rather

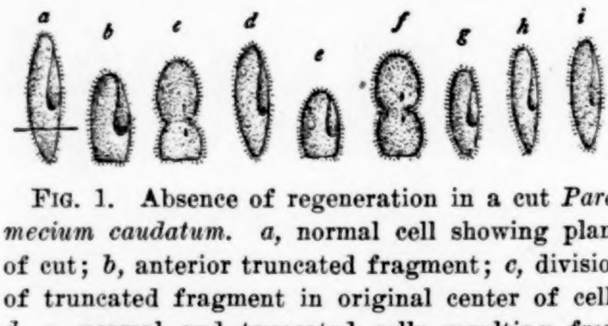

FIG. 1. Absence of regeneration in a cut *Paramecium caudatum.* *a,* normal cell showing plane of cut; *b,* anterior truncated fragment; *c,* division of truncated fragment in original center of cell; *d, e,* normal and truncated cells resulting from this division; *f,* division of second truncated cell.

than as causes, a more plausible explanation of the morphological relations of nucleus and cytoplasm is obtained. That excess of nucleus does not cause degeneration is shown by a simple experiment. If we cut *Paramecium caudatum* as shown in Fig. 1, *a,* the cut cell does not regenerate in the majority of cases, but divides in the original central plane of the organism (*b, c*). As a result of this division one normal (*d,* anterior) and one abnormal (*e,* posterior) cell results. The nucleus

divides equally as though the cell were perfect, hence the posterior cell has a reduced cytoplasm and a full size nucleus, or the ratio N/P is changed to the advantage of the nucleus. Nevertheless, this cell, in some cases at least, grows and divides again without regenerating the lost part and a second abnormal division (*f*) results in a second abnormal cell and a normal cell. Ultimately, however, the abnormality is lost and the normal form regained. Here, something more subtle than mass relations is at work and we are justified in looking for important results from the further study of protozoa along these experimental lines.

4. *Regeneration.*—The power of regeneration of the cell, also, is much less extensive than we were led to believe by the early experiments of Balbiani, Verworn, Gruber, Hofer, Prowazek and others. It seemed to follow from their experiments that any fragment of a protozoan, provided it contained some nuclear material, would regenerate quickly into a normal cell. Lillie showed that a piece as small as one twenty-seventh of the original animal would develop into a normal *Stentor.* The power to regenerate, however, varies not only in different races of the same species of protozoa, but also in the same cell at different inter-divisional ages. In four different races of *Paramecium caudatum* I have found that in one race only about one per cent. regenerated after cutting; in another about 10 per cent. regenerated; in a third race about 30 per cent. and in a fourth about 90 per cent. Here, then, is a well-marked racial difference in respect to regeneration.

Again, if we cut the large hypotrichous ciliate *Uronychia transfuga* just after division, both fragments will contain parts of the macronucleus, but only the micronucleus-holding fragment will regenerate. If cut from six to eight hours after division the result is the same, although the non-regenerating fragment lives for days. But if we cut the cell just prior to cell division, both fragments regenerate perfectly except for the absence of a micronucleus in one. The power to regenerate, therefore, varies in the same cell from a minimum just after division to a maximum just prior to division, a phenomenon lending support to the view that certain stuffs are accumulated during cell life up to a condition analogous to saturation, when the reaction follows, in this case regenerative processes. With such activity the accumulated stuff is used so that regeneration does not follow mutilation immediately after, or for some time after, cell division. Certainly the generalization that nucleated fragments of protozoa will regenerate is not well founded.

Similarly with other early generalizations. The classic experiments of Maupas seemed to prove that Weismann's theory of the potential immortality of protozoa was wrong. Later research confirmed Maupas in the main, until to-day Weismann's theory, in its original form at least, is untenable, protozoa having the same potential of immortality that metazoa have, no more and no less. Later research, however, has given highly variable results in studies of the life history, and again we find an individuality in different races of the same species. Woodruff's remarkable and enigmatical results with *Paramecium caudatum,* for example, show that earlier conclusions and generalizations were premature.

One general conclusion, however, seems to be well established, viz., that the protozoon's life history runs in cycles of asexual and sexual phases. The beautiful work of Schaudinn in 1899, on the life cycle of *Coccidium schubergi,* gave the model fol-

lowed by subsequent investigators in working out life histories of other forms, and there is no doubt now that the protozoon life cycle involves more or less definite asexual and sexual periods. In parasitic protozoa the sexual phase, including maturation, conjugation and fertilization, undoubtedly leads to renewed vigor of the race, or to a new power of asexual development, and to this extent at least, the time honored view of Bütschli's (1876) that conjugation is a means of the "Verjungung" or rejuvenation of the cell, is warranted.

Associated with these alternate phases in the life history are the remarkable changes which accompany development of the sexual phase. These, involving the problems of sex, are particularly important in connection with the nuclear changes whereby a specific germinal chromatin is formed, sometimes at an early stage, in the asexual phase, and persisting as a germ plasm until used in the formation of gamete nuclei.

I have now given enough of the scope of protozoology to indicate that the protozoologist, far from being a strict specialist, rather immodestly claims the greater part of the whole field of biology as his own, and I would define protozoology, therefore, as that branch of the biological sciences which deals with the application of biological problems to, and with search for their solution in, the lowest group of animal organisms—the Protozoa.

Gary N. Calkins

SYNTHETIC METALS FROM NON-METALLIC ELEMENTS[1]

It is one of the most striking facts of chemistry that three fourths of all the elements are metals. But it is no less remarkable that metallic properties are confined exclusively to elements in the free state or, in case of alloys, to combinations of typically metallic elements.

In recent years the theory of the nature of the metallic state has been steadily developing into more and more precise form, so that to-day we have, in the electron theory of matter, a very satisfactory explanation for all of the characteristic properties of metals. Inasmuch as it is just a century since Davy proposed his celebrated metallic ammonium theory, we may now well consider whether metallic properties are, of necessity, confined to elements in the free state.

During the last two decades a vast amount of experimental evidence has been accumulating that electricity is granular in structure, though such a conclusion was strongly indicated three quarters of a century ago by Faraday's discovery of the facts epitomized in the law of electro-chemical equivalents as first pointed out by Helmholtz in 1881. The granules or ultimate atoms of electricity are now called corpuscles or electrons. The charge of the electron is negative in sign. In fact we have decisive experimental evidence of only this one kind of free electricity, positive electrification of a body, being from this standpoint merely a deficiency of electrons.

J. J. Thomson has shown how from the conception of an atom made up of electrons rotating in a sphere of positive electrification, there follows a simple explanation of many of the properties of an atom, including valence; a univalent atom, if negative, being one that can gain an electron, if positive, one that can lose an electron. A bivalent can gain or lose two electrons. A trivalent atom, three, etc. According to this hypothesis the most fundamental property of an atom of an element is this

[1] Read at the meeting of the American Chemical Society, Minneapolis, December, 1910.

tendency to gain or lose one or more electrons. The tendency to lose electrons is greatest for the alkali metals and least for the noble metals. According to this view, for example, sodium and chlorine react with great energy because of the great tendency for each atom of free sodium to lose an electron, on the one hand, and each atom of free chlorine to take up an electron, on the other. The action consists, therefore, in the transfer of an electron from an atom of sodium to an atom of chlorine. The components of a molecule of solid salt are therefore not an *atom* each of sodium and chlorine, but an *ion* of sodium combined with an *ion* of chlorine, if by the term *ion* we now mean atom $\pm$ an electron.

The more or less complete "electrolytic dissociation" or "ionization" which occurs upon dissolving a salt in water is then due to the marked lessening of the electric force which holds together the ions of the solid salt by reason of the very great dielectric constant of water.

The application of the electron theory to the metallic state by Riecke, Drude, Lorentz, Thomson and others has led to results of the highest significance. Though the details of the relations of the electrons to the atoms are viewed somewhat differently by different physicists, it is however agreed by those who are working in this field that metals owe their most characteristic metallic properties of a physical nature to the *mobile* or free electrons which they contain. The absence of metallic properties in the solid non-metallic elements is, by this hypothesis, due to the supposed tendency of the atoms of such elements to *gain*, not lose, electrons: for which reason such a non-metallic solid will contain very few free or mobile electrons.

Thus, according to one view, electrons which are perhaps as numerous as the atoms of the metal, move about freely among the atoms, with which they are considered to be in kinetic equilibrium. Electric conductivity is then due to the drift of these electrons under the influence of the potential in the wire. Thermo-conductivity of metals is explained equally satisfactorily by the electron hypothesis. The calculated ratio of thermal to electrical conductivity and also the temperature coefficient of the ratio are in good agreement with the facts. Other metallic properties, including opacity to light, reflecting and radiating power, the Hall effect, the Thomson effect, the Peltier effect, etc., are equally well accounted for.

The most characteristic chemical property of a metal is its ability to form the positive ions of salts. Every true metal has this property well developed. If we electrolyze a solution of a salt, say silver nitrate, the free positive ions of silver are attracted and move toward the negative electrode; on coming in contact with which each ion has forced into it an electron, which converts it into an atom of silver. The aggregate of such atoms deposited on the cathode has metallic properties; owing to the great tendency of each atom to give up an electron.

When we come next to consider the behavior upon electrolysis of a salt of a compound basic radical, it is difficult to see wherein its behavior should differ from that of a salt of a metallic element. In this case, as in the other, positive ions are attracted to the cathode, and upon striking the latter can gain electrons. If then the electron theory of the metallic state is as fundamental as it seems to be, the aggregate of such free "neutralized" radicals should be a body having metallic properties; in other words, a "synthetic metal." For such a hypothetical body would be made up of radicals, which, analogous to

metallic atoms, could easily lose electrons. The mass would then contain an abundance of mobile or free electrons and in such case possess high electrical and thermal conductivity, metallic luster, etc.

Turning now from theory to facts, the case of ammonium amalgam demands consideration at once on account of its historical importance. This remarkable substance was discovered practically simultaneously and independently by Seebeck, and by Berzelius in 1808; curiously enough, in just the same year that Davy isolated sodium and potassium from their hydroxides. Two years later Davy, in 1810, compared ammonium amalgam with the amalgams of sodium and potassium and was led to announce his famous ammonium hypothesis; the radical ammonium was analogous to the alkali metals and was said to exist in metallic form, united with the mercury, in ammonium amalgam. Berzelius and Ampere also supported this view. Some years later, after the discovery of other radicals, Dumas and Liebig in a joint paper gave Davy's idea a much more general form. They wrote: "Organic chemistry possesses its own elements which sometimes play the part of chlorine or oxygen, sometimes, however, also, that of a metal. Cyan, amid, benzoyl, the radicals of ammonia, the fats, the alcohols and their derivatives, form the true elements of organic nature." But the hypothesis of the metallic nature of ammonium in the amalgam did not pass unchallenged. Gay-Lussac and Thenard concluded that the so-called amalgam is only a mixture of ammonia, hydrogen and mercury; a view subsequently shared by many others, among them Seely, who found the volume of the inflated mass to be inversely proportional to the pressure upon it. The case against the metallic ammonium hypothesis was made still stronger by the evidence furnished by an experiment by Landolt in 1868. If the amalgam is really analogous to sodium amalgam, if the radical actually has the properties of a metal, it should readily precipitate from solutions of their salts metals of smaller solution tension; but, in the test, Landolt could precipitate neither copper nor silver with ammonium amalgam.

The first really convincing evidence in favor of the ammonium hypothesis was furnished by LeBlanc in 1890. LeBlanc electrolyzed a solution of an ammonium salt with a mercury cathode. The apparatus was so arranged that simultaneous measurements of the polarization potential could also be made. This potential rose in a few minutes to a maximum which was nearly as great as that given by a sodium salt. The really important result, however, was observed after the polarizing current was cut off. The mercury cathode, which showed the inflation characteristic of ammonium amalgam, was still strongly electro-negative toward the solution and remained so for from ten to twenty minutes. That this effect was not due to hydrogen was shown by the fact that the hydrogen polarization potential was considerably smaller and that it fell off almost as soon as the current was interrupted. These experiments of LeBlanc, based as they were on the sound principles of electro-chemistry, gave a new impetus to the ammonium hypothesis. Coehn, in 1900, reasoned that if ammonium amalgam gave the high potential found by LeBlanc, it surely ought to precipitate copper and silver, and that Landolt's experiments should succeed. But Coehn failed exactly as did Landolt! Coehn next found that at very low temperatures, or even at zero, the amalgam was much more stable than at room temperature, and would precipitate copper from copper sulphate without difficulty.

This result was in itself insufficient to prove the metallic nature of ammonium, since free hydrogen was always present in the amalgam, and may have been the active substance in the reaction. To remove any doubt, Coehn then showed that not only are cadmium and zinc precipitated by the cold amalgam, but that barium amalgam results from the action at zero of ammonium amalgam on a solution of barium chloride. This fact was independently discovered later by G. M. Smith, who also obtained sodium and potassium amalgams in a similar manner. Thus the experiments of LeBlanc, Coehn and G. M. Smith furnish indisputable evidence of the metallic nature of ammonium in ammonium amalgam.

It is often stated that metals are insoluble in a physical sense, in all solvents excepting other metals. This statement can scarcely be upheld in view of the recent work of Kraus, on solutions of sodium, potassium, calcium, etc., in liquid ammonia. These very unique solutions, discovered by Weyl in 1864, seem to have many distinctive metallic properties. They are practically opaque, except when very dilute; even in this respect they resemble gold, which is transparent in very thin layers. They also show metallic luster, and reflection, and while they conduct the electric current, the conduction seems to be metallic rather than electrolytic in character. Upon evaporation, they deposit the pure metal in crystalline form. All such solutions, if sufficiently dilute, have a characteristic deep blue color. Now Palmaer has shown that by the electrolysis of tetraalkyl ammonium salts dissolved in liquid ammonia, unstable blue solutions are formed about the cathode. These blue solutions were thought to contain the organic radical in metallic form dissolved in ammonia. These facts have been confirmed by Kraus, who also concurs in the explanation.

LeBlanc's experiments on the polarization of mercury in solutions of ammonium salts, were also extended to include a similar study of salts of a number of substituted ammonias. Mono-, di- and tetramethyl and mono-ethyl ammonium ions gave results more or less like those of ammonium ions, from which facts LeBlanc concluded that in these cases also amalgams were formed, although none of the supposed amalgams were isolated. When attempts were made by Dr. Moore and myself to obtain an amalgam by the electrolysis of aqueous solutions of tetramethyl ammonium salts the results were complete failures; not a trace of amalgam could be isolated. But when we substituted absolute ethyl alcohol for water, as solvent, the amalgam resulted at once.[2] This amalgam differs greatly from ammonium amalgam in both appearance and stability. It is a crystalline solid of metallic luster, closely resembling sodium amalgam. It can be kept for days at temperatures below + 10 degrees and does not have any tendency to become inflated. Its density is somewhat less than that of mercury, but still many times greater than that of ammonium amalgam. Its electrical conductivity is comparable to that of a metal. Chemically, it resembles the alkali metal amalgams, but is far more active than that of sodium. It reacts with water with great energy and rapidity, giving hydrogen and the corresponding base, tetra-methyl ammonium hydroxide. From solutions of salts of copper and zinc, these metals are precipitated at once; while from solutions of salts of sodium and potassium the corresponding amalgams are formed. With solutions of ammonium salts the charac-

[2] McCoy and Moore, Science, **30**, 315 (1909); *J. Amer. Chem. Soc.*, **33**, 273 (1911).

teristic inflated mass of ammonium amalgam is produced.

The very high electrolytic solution tension indicated by these reactions is confirmed by direct potential measurements. The values obtained, for similar conditions, are about .6 volt higher than those found recently by Lewis and Kraus for sodium amalgam. This result is in harmony with the enormously greater activity towards water of the organic amalgam. Dr. Moore and I have also made mono-methyl ammonium amalgam and studied **its** properties.

The facts just discussed point clearly to the probability that in general positive ions, if free, or even amalgamated with mercury, will possess metallic properties. Practically, however, several causes may prevent the isolation of such metallic bodies. We know that it is not possible by electrolysis to separate many metals like sodium from aqueous solutions of their salts. Similar relations may obtain in the electrolysis of an organic salt. On the other hand, it is theoretically possible that such a compound metal may be so unstable in the free state that it suffers spontaneous decomposition at the moment of its formation from its ions. A third possibility is exemplified by the case of hydrogen. For a long time it was thought by some chemists that hydrogen in solid form would have metallic properties, since acids may be considered "hydrogen salts." The fact that solid hydrogen is now known to have no metallic properties proves clearly the fallacy of the old idea and seems to be also a flat contradiction of the hypothesis in question.

Now, hydrogen differs from the metals in one other important respect: while the molecules of metallic vapors are always monatomic those of hydrogen are diatomic. Thomson has considered the question of the theory of the union of two like atoms to form a molecule of an elementary gas, and has shown very convincingly that it is reasonable to conclude that one atom sends its valence electron into the other and that the combination is entirely analogous to that when two unlike atoms combine. If this is the case, it is possible to understand why solid hydrogen has no metallic properties; its valence electrons are *bound* and not *free* nor *mobile*. Analogously to hydrogen, some organic radicals which can form positive ions of salts may unite in pairs to form double radicals. These would not be expected to have metallic properties.

In some cases, however, even hydrogen seems to have some metallic properties; it dissolves readily in palladium and, when nascent, diffuses easily through iron. The latter property of hydrogen may be due to continued existence in the monatomic and therefore metallic state.

As I have tried to point out, the electron theory of the metallic state would lead us to expect that free radicals, formed by the neutralization of the positive ions of salts by the introduction into each ion of that number of electrons represented by its valence would have metallic properties. The facts just reviewed, though few in number, seem to me to lend support to this hypothesis, and to lead to the conclusion that it is possible to prepare composite metallic substances, which may be termed synthetic metals, from constituent elements, some of which at least are non-metallic.

HERBERT N. MCCOY

December 28, 1910

WILLIAM RUSSELL DUDLEY.

WILLIAM RUSSELL DUDLEY, professor of systematic botany in Stanford University, was born on a farm in North Guilford, Conn.,

on March 1, 1849, and died at Los Altos, Cal., on June 4, 1911.

The fact that the writer has been intimately associated with Professor Dudley since the day he entered the freshman class at Cornell University, in September, 1870, will perhaps excuse the personal element in this little sketch.

The word "instructor" as a technical term, describing a minor assistant to a professor, had just then been invented, and the present writer had just been appointed "instructor in botany" under Professor Albert N. Prentiss.

One day, Professor Henry T. Eddy, now of Minnesota, brought to me a tall, well-built, handsome and refined young man, older and more mature than most freshmen, and with more serious and definite purposes. Young Dudley had an intense delight in out-door things and especially in flowers and birds. He wanted to be a botanist, and had turned from old Yale, to which as a descendant of Chittendens, Griswolds and Dudleys he would naturally have gone, to new Cornell, because Cornell offered special advantages in science, and because at Cornell a good man could, if need be, pay his own way. For the rest of my stay at Cornell, Dudley was my room-mate, living in a cottage on the hill, built by students and termed "University Grove." In this cottage was established the boarding-club, known later and appropriately as "The Struggle for Existence," and in later and more economical times as the "Strug." For a time, Dudley paid his way by rising at four o'clock to milk cows at the farm. Later he was made botanical collector, and this congenial work he kept up until he became my successor as instructor in botany. In college Dudley was a member of the Delta Upsilon fraternity, and took an active part in holding this society to the high ideals (dikaia upotheke) on which it was originally based. He was also a charter member in the honorary scientific society of Sigma Xi (spoudon xunones).

In 1871 I went with him to his home at North Guilford, and I remember that his practical father said to me:

There comes Willie across the fields with his hands full of flowers, just as he used to. I wonder if there is any way he can make a living by it.

Dudley graduated from Cornell in 1874, with the degree of B.S. In 1876, he received the degree of M.S., after which he spent some time in botanical study in Strassburg and Berlin. From 1872 to 1876 he was instructor in botany at Cornell, his eminent knowledge of the eastern flora overbalancing the fact that at first he had not yet received a degree. From 1876 to 1892 he was assistant professor of botany at Cornell, with a year's absence in 1880, in which he served as acting professor of biology in the University of Indiana, in the absence of the present writer, who then held that chair.

In 1892, Professor Dudley became professor of systematic botany at Stanford University, which position he held until, in January, 1911, failing health caused his retirement on the Carnegie Foundation as professor emeritus, his work being then taken by one of his students, Associate Professor Le Roy Abrams.

Many of the leading botanists of the country have been students of Professor Dudley. H. E. Copeland, Kellerman, Lazenby, Branner were among his associates at Cornell. Atkins became his successor at Cornell. Abrams, Cook, Elmer, Olssen-Seffer, Cannon, Wight, E. B. Copeland, E. G. Dudley, Greeley, Herre, McMurphy and many others were under his tutelage at Stanford.

In Stanford University, Dudley was one of the most respected as well as best beloved members of the faculty. No one could come near to him without recognizing the extreme refinement of his nature; a keen intellect, an untiring joy in his chosen work, and the Puritan conscience at its best, with clear perceptions of his own duties to himself and a generous recognition of the rights and the aspirations of others.

Dudley entered with great joy into the study of the California flora. He became especially interested in the study of trees, the evolutionary relations of forms and especially the problems of geographical distribution.

The conifers of California were his special delight, and he made many field trips with his students to all parts of the state, notably to the Sierra Nevada and the Sierra Santa Lucia. His extended collections were presented to Stanford University, where with the collections of Dr. Abrams they form the major part of the large "Dudley Herbarium."

A genus of stone-crops, of many species, abounding on the cliffs of California and especially on those which overhang the sea, was named *Dudleya* by Britton and Rose. *Dudleya pulverulenta* is one of the most conspicuous plants in California wherever "sea and mountains meet."

Dudley was instrumental in inducing the state of California to purchase a forest of redwoods (*Sequoia sempervirens*), that this, the second of California's giant trees, might be preserved in a state of nature. Two thousand five hundred acres in the "Big Basin" of Santa Cruz County were thus bought and established as the "Sempervirens Park." For several years Dudley served on the board of control of this park.

Of the Sierra Club of California, Dudley was a leading member and for some years a director.

As an investigator, Professor Dudley was persistent and accurate, doing his work for the love of it. A partial list of his papers is given below. A large work on the conifers of the west was long projected, but still exists only in uncompleted manuscript.

Dudley was master of a quiet and refined but effective English style. He was one of those scientific men, too few I fear, who have real love for literature, and who understand what poetry is and what it is about. In his early days he wrote graceful verse. Three of his poems are in print, "The Kaaterskills as seen from the Taconics," "Sunrise on the Kaaterskill" and "A Legend of the Lehigh Valley." The last is the story of the Moravian settlements of "Friedenhütten, Tents of Peace, and Gnadenhütten, Tents of Grace."

From the first of these, I quote:

'Twas reached at last, with toiling long and weary
  Taconic's loftiest hill;
Then, vision of all visions, stood uncovered
  The domes of Kaaterskill!

They rose above the lesser hills as sovereigns
  Above the common herd;
They gathered then in conclave grand and solemn;
  They breathed no spoken word.

But full as anthemed voices of the ocean
  A soundless song was borne
Up from those lips that changeless through the ages,
  Sang on Creation's morn.

. . . .

A mighty calm sits on these silent summits,
  Time fades, as breath away,
O'er all in solemn oceanic pulsings
  Deep flows—Eternity.

From the "Legend of the Lehigh Valley," I quote the last verses:

Full six score years have passed away.
  Still on the silent summer morn,
At noon's repose, or evening's gray,
  O'er Lehigh's vale this dirge is borne.
    The reaper hears, on far-off hills,
    And traveler by the mountain rills,
    And fisher in the evening's chills;
They hear and feel some echo wake
  Of sorrow slumbering long. A tear
Is shed for some sweet lost one's sake,
  A tear that leaves life's stream more clear.
    They bless the song and them who sing;
    They feel the sympathy upspring
    That's born of human suffering.

The air is full of sad-toned bells
  That never cease their brazen toll;
With circling suns their pulsing swells,
  And in one tireless world-wave roll.
    But grateful unto sorrow's ear
    From the Lehigh, far or near,
    Comes this dirge so sweet and clear,—
    Come these human voices dear.

Professor Dudley's health was good until about three years ago, when he set out to study the trees of Persia. In Egypt he was attacked by a severe cold or bronchitis which ended in tuberculosis.

He was never married.

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David Starr Jordan

PROFESSOR WHITMAN'S COLLECTION OF PIGEONS

Biological investigators will be glad to know that the large and valuable collection of pigeons and birds which were the basis of nearly a score of years' work of the late C. O. Whitman are being maintained and kept together. The material upon which Professor Whitman's extensive evolutional and natural history investigations were made will thus be available while his manuscripts and records are being arranged for publication.

Very abundant material is at hand for a continuance of studies on hybridization, sex, fertility, instinct, etc., more than is now utilized to its full advantage.

Mrs. Whitman has arranged, as long as it is utilized, to keep together this material, priceless from its history, some of the birds having pedigrees reaching back for a long series of years. The collection has been gathered from all parts of the world, not only through long years which consecration to the work could alone have made possible, but also at great expense of money which could be made use of only through sacrifice. Those

who know best what this has been feel that the collection must be kept to be utilized as long as it will serve its purpose.

It was only in the last months of Professor Whitman's life that facilities for experiments and observation on a much larger scale were secured through the efforts of friends who put at his service the piece of ground adjoining his residence. He at once had built a large number of new cages; and delighted with the prospect of the enlarged opportunities declared that his real work he was just about to begin and that "five years will show."

For these reasons and because Professor Whitman's work became more illuminating as he went on, his family and friends feel that the opportunities so untimely left should be extended to others who wish them. Quarters are also given in the residence alongside the nearly one thousand birds, and Dr. Riddle, now at work with them, will cooperate with the work of others, or assist, or direct, as needed.

The library, which is one of the largest and most complete of biological libraries, is held open for constant use. The volumes are very extensively marked; pencil notes often bringing together from all quarters the various facts bearing on the subject under discussion.

SCIENTIFIC NOTES AND NEWS

The divisions of vertebrate and invertebrate paleontology and paleobotany in the U. S. National Museum have been combined into a new division of paleontology, with Dr. R. S. Bassler as curator in charge, Mr. J. W. Gidley as assistant curator of fossil mammals and Mr. Charles W. Gilmore, as assistant curator of fossil reptiles.

The presidents of the sections of the fifteenth International Congress on Hygiene and Demography to be held in Washington from September 23 to 28, 1912, are: (1) Hygiene microbiography and parasitology, Professor Theobald Smith; (2) Dietetic hygiene, Hygienic physiology, Professor R. H. Chittenden; (3) Hygiene of infancy and childhood and school hygiene, Dr. A. Jacobi; (4) Industrial and occupational hygiene, Dr. G. M. Kober; (5) Control of infectious diseases, Dr. Hermann Biggs; (6) State and municipal hygiene, Dr. Frank F. Westbrook; (7) Hygiene of traffic and transportation, Dr. W. Wyman; (8) Military, naval and tropical hygiene, Dr. H. G. Beyer; (9) Demography, Professor Walter J. Willcox.

The Paris Academy of Sciences has elected as foreign members Dr. Zaboudski, of St. Petersburg, in the section of mechanics, and Professor Perrincito, of Turin, in the section of agriculture.

We learn from the *British Medical Journal* that in recognition of Sir Patrick Manson's initiative in directing attention to the importance of the study of tropical medicine and of his work in that field of science, an international committee has been formed for the purpose of presenting him with a gold medal bearing his effigy. The medal is to be designed by Dr. Paul Richer, of Paris, who is eminent both as an artist and as a physician.

The annual meeting of the Society of Chemical Industry was held in July at Sheffield, under the presidency of Mr. Walter F. Reid. Dr. Rudolf Messel, of London, was elected president for the ensuing year.

We learn from the *Journal* of the American Medical Association that a meeting of the members of the Pennsylvania Pharmaceutical Association, Philadelphia Association of Retail Druggists, Philadelphia Branch of the American Pharmaceutical Association and its scientific section, Philadelphia Branch of the American Chemical Society, Philadelphia College of Pharmacy, etc., was held in Philadelphia on July 17 at which strong and vigorous protests were made against the suggested removal of Dr. Wiley; a preamble and resolutions were adopted and sent to President Taft, endorsing and commending Dr. Wiley's work and deploring any movement which would either cause Dr. Wiley to resign at this time or would tend to hamper him in his efforts to make the Food and Drugs Act effective, and thus practically render it a dead letter.

Dr. Leonhard Stejneger, head curator of the department of biology, U. S. National

Museum, has been designated as the representative of the Smithsonian Institution at the celebration of the one hundredth anniversary of the founding of the Royal Frederick University, to be held at Christiania, Norway, September 5 and 6, 1911. Dr. Stejneger will also represent the institution at the celebration of the five hundredth anniversary of the founding of the University of St. Andrews, which will occur from September 12 to 15, 1911. Dr. Stejneger is a graduate in arts, philosophy and law of the University of Christiania.

DR. A. S. PEARSE left the University of the Philippines on August 1, and has returned to the United States.

DR. M. C. SMITH, of Lynn, Mass., has sailed for Europe, where he expects to study the relation of the thyroid gland and the pituitary body to the development of the teeth and mouth, and to attend the meeting of the French Congress of Stomatology, and the meeting of the British Dental Society.

DR. G. W. CRILE, of Cleveland, Ohio, spoke to the physicians of San José, Cal., in the San José high school on July 7 on the subject of "Surgical Shock."

THE German central committee for cancer research has issued an appeal to create a foundation which is to be known as the Ernst v. Leyden foundation for cancer research and repression.

THE French Physical Society and other societies propose to collect a fund to honor the memory of the late M. J. Joubert, of the Pasteur Institute. The object of the fund is to found a scholarship, tenable at one of the institutions with which he was connected as pupil or teacher.

DR. JOHN BEDDOE, F.R.S., a practising physician who has made important contributions to anthropology, died on July 19, aged eighty-four years.

DR. W. SPRING, professor of chemistry in the University of Liège, died on July 17.

DR. CHARLES NÉLATON, vice-president of the French Surgical Society, died at his home in Paris on July 23.

BY the will of the late Charles S. Chase $100,000 is bequeathed to the Harper Hospital, Detroit. The income is to be used for the establishment of free beds and for the offering of prizes for research work looking toward the cure of cancer.

DR. L. D. MASON, of Brooklyn, vice-president of the American Society for the Study of Alcohol and other Narcotics, offers a prize of $150 for the best essay on "The Biological and Physiological Relations of Alcohol to Life." The essay must contain original research on the inherited effects of alcoholic degeneration. It must be sent to Dr. T. D. Crothers, Hartford, Conn., before July 1, 1913.

AN exposition of inventions, the first to be held in America, will open at St. Louis, on September 11. It is intended for the exhibition, demonstration and promotion of patented machines, appliances, devices, tools and processes of every character. Further information may be obtained from the manager, Mr. F. W. Payne, St. Louis Coliseum Company, St. Louis, Mo.

DURING the week of August 20–27, there will convene in Antwerp, Belgium, the seventh International Esperanto Congress, in which over 2,000 delegates from every country of the world, will take part, all using one common tongue, the international auxiliary language Esperanto. In addition to the regular Esperanto delegates, several nations will be represented by official government delegates. Mr. Edwin C. Reed, of Washington, secretary of the Esperanto Association of North America, has been apppointed by the Secretary of State to represent the United States.

WE learn from *Nature* that a meeting of the British Institution of Mechanical Engineers was held on July 25 and 26, at Zürich in the Swiss Polytechnikum. Among the papers on the program were: "Electric Traction in Switzerland," by Mr. E. Huber-Stockar, of Zürich; "Results of Experiments with Francis Turbines and Tangential (Pelton) Turbines," by Professor Franz Prášil, of Zürich; "Some New Types of Dynamometers," by Dr. Alfred Amsler, of Schaffhausen; "Rackrail-

way Locomotives of the Swiss Mountain Railways," by Mr. T. Weber and Mr. S. Abt, of Winterthur; "High-pressure Water-power Works," by Mr. L. Zodel, of Zürich.

The preparations for the Australian Antarctic expedition are, Reuter's Agency learns, practically completed, and the expedition ship *Aurora,* under the command of Captain Davis, left the Thames at noon on July 25. Only two members of the staff of the expedition will go out in the ship—viz., Lieutenant Ninnis, assistant surveyor, and Dr. Mertz, zoologist. All told, the officers and crew of the *Aurora* will number 25; they will throughout remain with the ship. The bulk of the stores for the expedition is going out by the *Aurora,* which will also take the 48 dogs secured in Greenland, 30 sledges built in Norway and a very extensive oceanographical equipment which has been lent to the expedition by the Prince of Monaco. The monoplane built for the expedition by Messrs. Vickers is now being tried at Brooklands, and will be sent by mail steamer to Australia.

A cablegram from Sydney to the London *Times* reports that one of the northern territory scientific expeditions has left the railway at Pine Creek *en route* for the Roper River. It formed an imposing cavalcade, with its four heavily laden buckboards, 50 horses, donkey-wagon and team, and many camp attendants. Professor Baldwin Spencer has made valuable observations on the natives of the Adelaide River Plains and Melville Island. Mr. Gilruth, the microbiologist, has shot buffaloes, all free from the cysts found in Australian beef; Dr. Breinl, the expert in tropical diseases, has been testing the blood of white children of the third generation. Dr. Woolnough has examined the geological features of the mining fields. Another expedition under Captain Barclay is safe at Newcastle Waters, and intends to follow the course of the McArthur River to the Gulf of Carpentaria, opening up stock routes.

The work of the U. S. Geological Survey shows no lessening in conservation activities. During the month of June the land-classification board of the survey appraised 174,910 acres as coal land in the western states, placing a valuation thereon of $3,239,369. At the minimum price under which these lands could have been sold before the present regulations were in force, this same area would have brought only $1,392,179. During June the survey also classified 1,415,415 acres as noncoal land and transmitted the findings to the General Land Office so that the land might be restored to the public domain. These classifications were based on the field examinations of the survey geologists. There have now been classified under the present administration, in 40-acre tracts, 16,873,370 acres as coal land, with the valuation of $711,992,537. The valuation of these same lands at the minimum prices would have been $266,652,431, the difference in favor of the government under the present policy being therefore $445,340,106. During the same period 39,215,844 acres have been classified as noncoal land and restored to the public domain. In June three new withdrawals of supposed coal land were made in North Dakota and Wyoming, embracing 714,923 acres, and four restorations were made in Idaho, Montana, Utah and Wyoming, aggregating 1,847,264 acres. In connection with the classification of oil land, the secretary of the interior withdrew in June 170,333 acres in Wyoming, making a total of 3,970,429 acres of oil land now standing withdrawn and awaiting needed legislation in the interests of the conservation of the nation's extensive petroleum deposits. In administration of the phosphate lands 149,129 acres found not to contain phosphate deposits were restored to entry, leaving a total of 2,399,416 acres of phosphate land standing withdrawn and also awaiting necessary legislation. The tonnage of these important deposits has been conservatively estimated at over two billion tons of high-grade phosphate rock. Of land available for the development of water power 10,019 acres were withdrawn during the month by the secretary of the interior, making a total of 1,515,423 acres, including thousands of water-power sites, standing withdrawn in aid of proposed legislation which shall allow for

their development and yet protect the interests of the public.

THE *Medical Record* states that the specimens added to the Hunterian Museum during the last twelve months include an important collection illustrating cancer in mice, presented by the Imperial Research Fund. Sir Henry Butlin has given his drawings showing the appearances of this disease and conditions which may be mistaken for it. The tuberculosis commission has given a series of specimens showing the experimental production of that disease. Additions have also been made to the pathological and gynecological collections.

UNIVERSITY AND EDUCATIONAL NEWS

MR. JOHN G. ARCHBOLD has made a further gift of $40,000 to Syracuse University.

GOVERNOR DIX has approved two bills passed by the recent New York Legislature, one appropriating $140,000 for the Oswego Normal School, and one appropriating $50,000 for an Agricultural College, Cobleskill.

UPON recommendation of President Hutchins, of the University of Michigan, a committee consisting of three members of the board of regents, and five members chosen from the literary, medical and engineering faculties, has been appointed to make recommendations concerning a thorough reorganization of the graduate school. Also upon the president's recommendation, a resolution has passed the board of regents which makes it the duty of the committee of the board for each department acting with the president and the dean of the department to examine into the record of each member of the teaching force not later than February of each year "with a view of ascertaining what each member of said force is accomplishing in the field of his specialty, and as to the effectiveness of each as an instructor. The object is that the board may have the data for its guidance and information in regard to the question of promotion and retention in service of the members of the different faculties."

THE medical school in Shanghai will open its doors in February, 1912, under the auspices of the Harvard Medical School. Dr. Martin R. Edwards will be the head and will have a corps of fifteen assistants, most of them Harvard graduates.

THE following changes in the teaching force of the medical department of the University of Pennsylvania are announced in the *Journal* of the American Medical Association: Dr. G. T. Thomas to be associate professor of applied anatomy; Dr. George William Norris to become assistant professor of medicine in place of the late Dr. Aloysius O. J. Kelly; Drs. Joseph Rex Hobensack, William E. Quicksall, Penn-Gaskell Skillern and Nate Ginsburg to become assistant demonstrators in anatomy; Dr. Oscar H. Plant to become demonstrator of pharmacology; Dr. James H. Austin, demonstrator in pathology, to become associate in research medicine; Dr. A. B. Eisenbrey, associate in research medicine, to become instructor in surgery; Dr. James S. Hickey, absent on leave, will resume duty as assistant in physiology.

PROFESSOR J. I. D. HINDS, of the Peabody College, Nashville, has been elected professor of chemistry at Cumberland University.

DR. HERMAN M. ADLER has been appointed instructor in mental diseases at the Harvard Medical School, and will be no longer officially connected with the department of pathology or neuro-pathology. He will retain the position of pathologist at the Danvers State Hospital.

AT the New Mexico College of Agriculture and Mechanic Arts Mr. H. S. Hammond, assistant professor of biology, has been advanced to be associate professor and acting head of the department to fill the vacancy caused by the resignation of Professor E. O. Wooton, who enters the government service. Mr. D. E. Merrill, of the State University of Iowa, has been appointed assistant professor in the department.

MR. AUGUSTUS L. BARKER, M.Sc. (Alabama), has been appointed instructor in biol-

ogy in the University of Alabama to take the place recently vacated by Mr. James J. Durrett.

Dr. F. B. Dains has resigned the professorship of chemistry in Washburn College to accept an associate professorship of chemistry, in charge of organic chemistry in the University of Kansas.

Professor Karl Pearson, F.R.S., has been appointed to be the first occupant of the chair of eugenics in the University of London, established by the legacy bequeathed for that purpose by the late Sir Francis Galton.

Dr. Erhard Schmidt, professor of mathematics at Erlangen, has been called to Breslau.

DISCUSSION AND CORRESPONDENCE

THE AIR WE BREATHE IN BUILDINGS

To the Editor of Science: In a recent number of Science Dr. Gulick asks several questions with regard to the behavior of aqueous vapor in the air, and particularly as to the reason why air when heated becomes drier. All of his questions could be answered by any competent physicist, or could be resolved by reference to any good text-book of physics or of meteorology. But unfortunately, in these days of over-specialization, the language is apt to be too technical, or in the text-books the information too scattered, to be readily found and comprehended by the general reader. Hence the following explanations may be of some use to him and others in a like position.

There are two popular misconceptions, which it is necessary first to dispel. To begin with, few people seem to understand why water is wet. They think, moreover, that because *water* is wet, the same is true of ice and of aqueous vapor. Now this is not the case. Both ice and aqueous vapor are themselves *dry*. They *become* wet, only when they turn to water, ice when it melts, aqueous vapor when it condenses. Hence of the three water is alone wet, and all real moisture is due to the presence of water. So dry is aqueous vapor that it will dry any moist object that it comes in contact with, just as would superheated steam or a dry gas, which in fact are only other names for the *same* thing. Only, we give the name superheated steam to the vapor when the temperature and pressure are much above those of the atmosphere, as in the case of a steam boiler. Of course we must distinguish between the vapor itself, which is a true gas, dry and transparent, and the cloud or mist into which it condenses, on issuing from a locomotive. Hence it is, strictly speaking, incorrect to talk of the moisture or humidity in the air. There never is any moisture or humidity in the air, unless it be such cloud or mist. The described fallacy therefore consists in identifying things which are different, and distinguishing things which are the same—identifying moisture, humidity and water vapor—and distinguishing water-vapor, superheated steam, and dry gas—which are the same.

The second misconception consists in speaking of the *air* as moist or dry—an error not likely to be dispelled by the language of the text-books, which include sections on the "Hygrometric Condition of the Atmosphere." Dr. Gulick falls victim to this misconception when he seeks to explain the apparent drying of the air on heating as due to some *action* of the air on the contained moisture. Thus he says (p. 327), that on heating the air from 32° to 70°—"It appears that one of two things must have happened—either the heat must have contracted the existing moisture or the capacity of the air for moisture has been vastly increased by the rise in temperature." As a matter of fact neither happened, and, moreover, the air had nothing whatever to do with the matter. The same thing would have occurred if the air had been entirely absent, the aqueous vapor alone present. That is to say, aqueous vapor which at 32° seemed relatively moist, would become apparently drier if heated to 70°, whether the space filled by it were simultaneously occupied by air or not. This independence of the substances was first deduced theoretically by Dalton, afterwards established experimentally by Regnault, at least with a high degree of approximation. Hence it is a change in the condition of the aqueous vapor, not of the air, to which the

apparent drying is due, and it is the nature of this change which I must now endeavor to explain.

I have said that aqueous vapor is always dry. How then can it be at times *apparently* drier than at others? The reason is that we judge of the wetness or dryness of a place by the rate at which evaporation occurs therein. This depends upon the elevation of the temperature of the vapor above its dew-point, or that temperature at which it would condense. Suppose we had a hollow vessel enclosing a perfect vacuum. Now introduce a small amount of aqueous vapor at 32° temperature. The vapor will immediately expand until it fills the whole space, and by the heat vibration of its molecules will exert a certain pressure against the sides of the vessel. If now we introduce some more vapor, the latter will likewise expand and the pressure will be increased. But at 32° the vibrational energy of the molecules is limited. If we keep on adding vapor we shall presently so increase the density that this energy can no longer keep the molecules separate. Some of the vapor will condense. There is then a maximum density or pressure, which, so long as the temperature remains at 32°, can not be exceeded. The vapor is then said to be saturated. Suppose, when we arrive at this point, we raise the temperature to 70°. The heat energy of the molecules is thereby increased, and we shall find that we can now put in considerably more vapor before the limiting density and pressure are reached. Hence the latter rise with the temperature, or, what is the same thing, the dew-point or boiling-point increases with the pressure. Now a moist body must be considered as a source of aqueous vapor. If such a body is put into our saturated vapor at 32° no evaporation from it will take place. If the temperature is raised to 70° the vapor becomes superheated, and more vapor is required to saturate it. The moist body becomes the source of that vapor. Evaporation takes place the more rapidly, the greater the degree of superheat, or in direct proportion as the amount of vapor actually present is in defect of that required for saturation. The ratio of the former to the latter is technically known as the *relative humidity*. Thus the apparent dryness of a place depends solely upon the condition of the aqueous vapor therein, and not at all upon that of the air.

Whenever air is heated for a building it should be moistened; whenever cooled, it should be dried. This is generally appreciated, but unfortunately the arrangements provided are usually inadequate. A very considerable amount of moistening is required. The Sturtevant Company, however, manufactures a heating apparatus in which steam is blown into the hot air current from one of their fans, by means of a nozzle which finely atomizes the steam. This insures good mixing. It is found that with such an apparatus a much lower temperature suffices for comfort, and is also more healthful. In Europe, where the winters are, in general, moister than ours, lower house temperatures are habitual. I spent three winters in Italy, and can vouch for the fact that when the temperature in my study reached 65°, I found it uncomfortably warm. A certain Italian lady, who considered 55° in her own country a comfortable temperature and 60° too warm, finds 70° in this climate insufficient.

The fact that air when cooled increases in dampness is much more noticeable. It is a serious impediment to the use of refrigerating apparatus for cooling houses. Air which at 90° has a relative humidity of only 65 per cent. becomes saturated at 70°. If air thus cooled were admitted to a room, moisture would condense on the walls. Such conditions would naturally be very disagreeable.

Though the above explanations are only a rehash of well-known principles, I hope they may be of some use. In return I wish some one would explain to me just what is the danger of the open window. Why is a little sneaking draught in the house a source of colds and grippe, while a high wind out-of-doors a pleasure and a benefit? This is a problem that has long puzzled me, but perhaps it is a foolish question.

M. Mott-Smith

Waterville, Me.

THE MOISTURE IN THE AIR WE BREATHE

DR. GULICK's letter about the air we breathe in buildings, in the March number of SCIENCE, calls attention to some difficulties that have been troublesome to many of us for a long time.

During the winters of 1896–7 and 1897–8, I made a series of observations in office, residential and school buildings in Milwaukee, Wis., giving particular attention to the humidity during the period of artificial heating. The results of this preliminary study were published in a condensed form in U. S. Weather Bureau Bulletin, No. 24, in 1899. Later observation and study tend to confirm most of the conclusions reached at that time, but have failed utterly to furnish a satisfactory answer to that most important and all-including question, why in-door living is less healthy than out-door living. Certainly, there is some condition of environment, inimical to health, seemingly brought about by artificial heating that, thus far, has escaped observation.

The most obvious difference between inside and outside air appears to be in the moisture content, and, as Dr. Gulick asks a number of pointed questions about this important constituent of the atmosphere, a non-technical statement of the generally accepted view may be of interest.

Unfortunately the terminology used to express various conditions of atmospheric moisture was invented before we knew as much as we now think we know about the several factors involved, and, therefore, instead of assisting to a proper understanding tend to confusion.

1. The expression, capacity of air for moisture, is misleading. A better expression is, capacity of space or vapor for moisture, because the presence of air in space has nothing whatever to do with the capacity of the space for moisture, the only effect of the presence of air being to retard the diffusion of moisture within the space.

2. Likewise, the expression, saturation of air, implies that the presence of air affects the amount of moisture required to saturate a given space, which is not the case. It is a rather curious fact that, although atmospheric air is a mixture of nine or ten different gases, each gas, including vapor of water, tends to arrange itself according to its density and acts in all respects as it would if no other gas was present. In other words, each gas forms an atmosphere about the earth independent of all other gases. We, therefore, may eliminate dry air from consideration because it is not a factor in the problem.

To assist in obtaining a definite view let us imagine a cylinder of space 50 feet in diameter extending upward from the surface of a lake a distance of 10 miles, which is about as high as vapor will rise to an appreciable extent in our atmosphere. We will assume that the average temperature of the space within the cylinder is 40° F., and that the temperature of the surface of the lake is the same. How does the water in the form of vapor pass from the lake into this space? The molecular theory of matter teaches that the molecules of water are in a constant state of agitation; that the velocity and amplitude of their vibrations varies with the temperature, being greater for high temperatures than for low temperatures; and that some of the molecules in darting about attain a velocity and direction that carry them beyond the attraction they have for each other, and, hence, they fly off into the space above the water. This is our understanding of the process of evaporation. But it so happens that these molecules of water in the form of vapor do not escape the control of gravity, which operates to pull them down toward the earth in accordance with their weight or density. As the molecules continue to escape and a greater number pass into the space, they impinge more and more upon the surface of the water and increasingly impede the escape of other molecules from the surface, so that the process of evaporation becomes slower and slower. It, however, does not cease entirely, because the molecules of vapor also are in a constant state of agitation, and, in darting about and beat-

ing upon each other, some are carried into the water by their own velocity and some are thrown into the water by the force of the blows received from other molecules, thus decreasing the number in the vapor and allowing others to escape from the water. When the number that escape and the number that are carried and thrown back into the water equal each other a condition of equilibrium is established and the space is said to be saturated.

If the temperature of the space or the vapor within the space now be raised, what will happen?

The molecules of vapor at a temperature of 40° F. have a given velocity and amplitude of motion. The increase of the temperature from 40° to 50° increases their velocity and movement, and to exercise this increased activity requires more space. We, therefore, are accustomed to say that the vapor expands or increases in volume when its temperature is raised. In expanding some of the vapor overflows the original space, and the number of molecules within the space is thus decreased by the number that has been crowded out of the cylinder. This destroys the condition of equilibrium and permits the molecules at the surface of the water to escape again in greater numbers. Thus, the process of evaporation continues, establishing finally as before a condition of equilibrium at the new temperature of 50° F. This is our understanding, why increased temperature gives increased capacity when the vapor is free to expand, except for the control of gravity. But if we put a lid on the cylinder and thus confine the vapor to a definite space we limit the field of its activity but not the activity itself. The effort of vapor, humidity, steam, water gas—whatever name we may use to designate it—to obtain more space increases with its temperature whether confined within a limited space or not. If the space is limited the effect is increased pressure; if not limited increased volume. In either case it obeys the laws of gases. The only difference between atmospheric moisture and steam is that the activities of the former are limited by gravity alone, while the activities of the latter are confined to a definite space.

Wilford M. Wilson
Cornell University

A VARIANT IN THE PERIODICAL CICADA

While collecting material for a study of the mode of pigment formation in the periodical cicada (*Tibicen septendecim* L.) my attention was attracted by an adult male in whose eyes the red pigment was lacking. The specimen was secured at Summit, N. J., on June 6, and although the cicadas occurred there in countless thousands I searched in vain for a second specimen.

The example secured differed from the usual form not only in lacking the red pigment of the eyes, which in this specimen are perfectly white, but also in the coloration of the wing veins. In this individual the costa of the fore wings and the costa and the greater portion of the radius and media of the hind wings lack the typical orange coloration and are perfectly colorless.

Morgan[1] has recently caused white-eyed mutants to occur in *Drosophilia* by closely inbreeding and it may be that this specimen originated in the same manner if we assume that the entire colony is descended from one pair of cicadas. A study of the inheritance of this trait would be very interesting, but such a study is obviously impracticable owing to the long period of adolescence.

Ross Aiken Gortner
Station for Experimental Evolution

QUOTATIONS

TRIPPED BY RED TAPE

That the Department of Agriculture should be in danger of losing three of its leading experts on food adulteration, Wiley, Bigelow and Rusby, on account of a technical violation of the salary regulations, shows how a government is hampered by its bureaucratic methods. It is not claimed that Professor Rusby, of Columbia University, was avaricious in refusing to work for $9 a day or that the departmental authorities who arranged for him to be paid at the rate of

[1] Morgan, Science, N. S., XXXIII., No. 849, p. 534, April 7, 1911.

$20 for part of the year were wasting public funds. Any one of these three men, if he had been willing to put his knowledge of chemistry at the service of an adulterator of food or an evader of customs, could have made a great deal more money and had a much easier time of it.

It is not merely a pecuniary sacrifice which must be made by men of exceptional ability and proficiency when they enter any branch of government employ. A greater deterrent is the fact that they find that they are not free to work in their own way but have to submit to the detailed dictation of a lot of clerks and lawyers. This is particularly the case with the scientific departments. The scientific temperament is in eternal conflict with the legal temperament. The one cares only for results; the other insists upon methods. The former is striving for something new; the later sticks to precedents. Consequently the scientific men in government employ are apt to be in a chronic state of irritation unless they are of the conventional routine type of mind, that is to say the unscientific type of mind. In the case of a high spirited and original genius this irritation sometimes rises finally to the pitch of exasperation and he goes off on a tangent, sending in a farewell letter to "the department" telling them just what he thinks of them for refusing to pay for that tin cup which he bought without the proper requisition or for sending back his last report because only one color of ink was used on it. Men of calmer temper will get along somehow rather than give up work they are interested in, paying for the things that are necessary but not allowed, out of their own pockets, or collecting money on the side from some patron of science, and resorting to various evasions and misclassifications to get within the letter of the law. Probably the strict and literal enforcement of all the regulations in any department would stop its work. We have experimental evidence in support of this supposition, for in France and Italy it has been tried in the government railroad and postal service, where the employees instead of striking decided to obey the rules, all of the rules, all of the time. The result proved that obedience was better than sacrifice of wages because it was more effective in tying up the traffic.

The United States government has been remarkably liberal in its appropriations for scientific purposes, both theoretical and practical, but the results have not always been commensurate with the expenditure, partly because of the conditions under which the work had to be performed. By a process of natural selection the men of greatest initiative and originality tend to be eliminated out of the system. This is why the phrase "Washington science" is so commonly used in a derogatory sense.

Now the Bureau of Chemistry, under Harvey W. Wiley, for the past twenty-eight years has succeeded in keeping out of the ruts. It has set a fast pace for the state agricultural experiment stations. It has made many original contributions to science. It has initiated many valuable reforms in legislation and in agricultural practise. Dr. Wiley has a good temper. He laughs and grows fat on worries and opposition that would drive some men mad. He has been able to live in a bureaucratic atmosphere without losing his scientific spirit, or, what is more remarkable, his zeal for reform.—The *Independent*.

DOCTOR WILEY

(With apologies to Rudyard Kipling)

''What makes the Potted Ham so green?'' said Files-on-Parade.
''It's feelin' fresher than it is,'' the Color Sergeant said.
''What makes the ranks so white, so white?'' said Files-on-Parade.
''They're dreadin' what they've got to eat,'' the Color Sergeant said.
''For, they're bouncin' Doctor Wiley, you can hear the Microbes cheer,
And the Germs is all a-singin' 'Wiley's goin' away from here,
And we're comin' back far stronger than we've been for many a year,
For they're bouncin' Doctor Wiley in the mornin'.' ''

. . . .

"For what do they be bouncin' him?" said Files-on-Parade.
"'E put the Microbes on the blink," the Color Sergeant said.
"An' did the Microbes 'urt the Blink?" said Files-on-Parade.
"They put the Blink out of a job," the Color Sergeant said.
"They are bouncin' Doctor Wiley, and the germs are runnin' free,
And the Microbes an' Bacilluses are chortilin' with glee,
For they'll get their starvin' 'ooks once more on folks like you an' me,
After bouncin' Doctor Wiley in the mornin'."

—Horace Dodd Gastit, in *Harper's Weekly*.

SCIENTIFIC BOOKS

Handbuch der Klimatologie. Von Dr. Julius Hann, Professor an der Universität Wien. Dritte Auflage. 3 Volumes. Prices 15; 15; 23 Marks. Stuttgart, J. Engelhorns Nachf. 1908, 1910, 1911.

A laborious work is now completed and published. The progress of science may years hence suggest modifications and improvements. The history of science may bring into prominence the names of others than those quoted in this great work, but for the present this monument must stand alone, towering over other books as the pyramids of Gizeh tower over the valley of the Nile.

For forty years past Dr. Julius Hann has been filling meteorological journals and literature with a steady stream of works on the subject that has absorbed his thoughts and life. Neither Newton nor Laplace surpassed him in intense concentration of effort; neither Euler nor Humboldt have published more voluminously. Neither "The Voyage of the *Challenger*" nor all the polar expeditions of the past thirty years have contributed more to our accurate knowledge of the atmosphere of our own globe.

In three volumes of text totalling 1,400 octavo pages "The Founder of Modern Climatology" has given us both numerical and textual descriptions and comparisons covering all the characteristic features, both the general and the special local characteristics, of all the known climates of the globe. At first sight it would seem impossible to do this; but at numerous localities the forces that build up local climates are the same, so that the relative importance of one or the other force controls the result.

Complex as are the atmosphere and its relations to the earth and man, to geology and biology, to history and religion, yet all can be analyzed into temperature, moisture, sunshine and wind. The tabulation of these fundamental data gave Hann the handy material for statistical intercomparison and study. Hence his volumes are crowded with facts—dry facts, if you will, but reliable material for careful study. Of course the popular writer, the superficial traveler, the advertising land owner, is satisfied with a few striking items; but the careful engineer, the large planter, the discriminating physician, need every possible detail that can affect any feature of human interest. It is for these and all other accurate students that Hann has compiled these solid volumes. The exhaustive range of his reading, the continuous appeal to the pencil memoranda that he must have kept, the quotations of reliable figures instead of general verbal descriptions, make one feel that here we have condensed facts and not fancies. Even the elusive "sensible temperature" or "curve of comfort" or the sensation of temperature seems reducible to an exact function of temperature, humidity and wind.

Of course no satisfactory résumé of Hann's "Climatology" can be given here. We need only say that volume I. (Stuttgart, 1908) is the revised edition of an earlier work, translated and published in 1903 by Professor R. DeC. Ward, of Harvard University.

The second volume (Stuttgart, 1910) deals with the tropical zone or the whole region between the tropics of Cancer and Capricorn. This is one half of the whole globe and in some respects the most important half; it extends from New Orleans, Cairo, Bagdad, Hong Kong, Hawaii, and the Bonin Islands on the north, to Peru, Bolivia, Paraguay,

Madagascar, Java on the south. Therefore it includes the Amazon, Nile and Niger, Zambesi and the rivers of India. The third volume treats of the north and south temperate zones and finally, also, the Arctic and Antarctic regions. The latter are small in area but exceedingly interesting in their mysteries. The temperate zones are those on which we love to dwell because there the events of the modern world have taken place. Of course the six hundred pages of Hann's third volume, devoted to the temperate zones, and the following one hundred, devoted to the Arctic and Antarctic regions, are full of interest and novelty. Although we think our annual means of temperature are moderate and temperate as compared with those of the polar regions and the tropic zones, yet the monthly means and the annual ranges show the greatest contrasts. Thus we may have $-40°$ C. and $-50°$ C. as the normal for an average January in northeastern Asia, while the same Januaries in southern Asia, in Japan and China, Afghanistan, Persia, may be from zero up to $+10°$. By analogy we find the January temperatures in North America show similar contrasts, such as $-14°$ C. at Bismarck, N. Dak., and $+12°$ C. at New Orleans, or 10° C. at San Francisco and $-5.6°$ C. at Portland, Me. Such contrasts of average temperature give zest to life in the temperate zones.

CLEVELAND ABBE

Resultats du voyage du S. Y. Belgica en 1897, 1898, 1899, sous le commandement de A. de Gerlache de Gomery: Océanographie, les glaces, glace de mer, et banquises par HENRYK ARCTOWSKI; Schizopoda and Cumacea by H. J. HANSEN, 1908; Diatomées par H. VAN HEURCK; Petrographische untersuchung der Gesteinsproben von A. PELIKAN; and Quelques Plantes fossiles des terres Magellaniques par A GILKINET. 1909.

Still another batch of publications on the results of the *Belgica* expedition to the Antarctic is at hand, and more to come, according to the schedule, though one can not help wondering if volume X., by Dr. F. A. Cook, will eventually be among them, as originally announced. In the present instance the work is by scientists of quite another stamp.

In his discussion of the different forms under which ice appears in those regions Arctowski attempts to systematize and sum up the data given much more fully in his journal of the voyage; and also to consider the question of the limits of the ice pack, historically and from the *Belgica* observations. The movements and behavior of the pack and floe ice are fully explained. The character of the surface and how it is affected by wind and snowstorms are admirably shown by excellent half-tone reproductions of photographs.

Hansen devotes 20 pages and three excellent plates to the study of the crustacean Euphausia, Cyclaspis and related forms of marine habitat, so characteristic of the austral seas.

Van Heurck treats of the diatoms obtained in samples of the bottom obtained in sounding and in the residue from melted sea ice obtained at various places. The diatoms of the plankton are reserved for further study. An appendix on the diatoms of Kerguelen and a complete list of polar diatoms, Arctic and Antarctic, complete the memoir which is illustrated by thirteen phototype plates whose execution leaves nothing to be desired.

The photography of the rock specimens brought home by the expedition is the subject of Pelikan's memoir. The rocks are crystalline or igneous, mostly granite, diorites, porphyrites, basalts and gangue minerals. Two plates of magnified microscopic sections accompany the text.

Gilkinet devotes a few pages to a few fossil plants, mostly beeches and *Myrtiphyllum,* not new, but which present a certain interest because they come from a station near Punta Arenas, not far from a locality visited by the Swedish expedition, and comprise species not hitherto known from that locality, but only from the Sierra de los Baguales at a considerable distance from the *Belgica* locality. Also the maps showing geological distribution have not indicated hitherto tertiary beds at

the *Belgica* locality which is Passo del Cabeza del Mar, near Pecket Harbor, Strait of Magellan.

The members of the *Belgica* expedition are to be congratulated on the quantity as well as the quality of the results of their arduous labors in the field.

Wm. H. Dall

The Subantarctic Islands of New Zealand. Reports on the geophysics, geology, zoology and botany of the islands lying to the south of New Zealand. Philosophical Inst. of Canterbury, Wellington, N. Z. Government printer. 1909. 848 pp., 4to, plates, text-figures and maps.

New Zealand is situated upon a submarine bank, roughly twenty degrees of longitude wide and twenty-five degrees of latitude long in a north-and-south direction within the 1,500-fathom curve. The islands of which this report treats, with the exception of the Macquarie group, are included within the 1,000-fathom curve together with the north and south islands of New Zealand proper. The most important groups are those of the Chatham, Bounty, Antipodes, Campbell and Auckland Islands. Only Macquarie and Campbell are within the northern limit of drifting ice, but the curve-enclosing sea bottom less than 2,000 fathoms in depth indicates a connection between the neozelandic bank, the Antarctic lands and Australia by way of Tasmania.

The climate of these islands is cold, wet and tempestuous, their coasts in large part inhospitable, with projecting reefs and dangers; and the record of shipwrecks and loss of life, or extreme privation of survivors, is most melancholy. Into these perilous waters the search for the fur seal and sea elephant drew many adventurers, a goodly number of whom hailed from the United States; and, while occasional fortunes were made, many ships and men suffered disaster.

The government of New Zealand has established depots of provisions and other necessaries on the principal islands, for the relief of shipwrecked mariners, and once a year the government vessel makes the round of the islands to supply or repair these depots and rescue any persons who may have reached these desolate shores. On the petition of the scientific societies of New Zealand, the authorities agreed to transport an exploring party to Auckland and Campbell Islands and to pick them up on the return trip in 1907.

The collections and observations thus made form the basis of two handsome volumes, consecutively paged, profusely illustrated, and edited by Professor Charles Chilton, of the University of New Zealand.[1] The government of New Zealand contributed a substantial sum toward the expenses of publication.

The fauna and flora of these isolated islands, seldom visited by man and into which only a few pests like rats and mice from whaling ships or sealers can have been unintentionally introduced, have a very special interest, not only on account of the modification the plants and animals have undergone, but for the light they may throw on the former distribution of Antarctic lands.

It is impossible within the space assigned to us, to discuss the several papers by specialists which are brought together in these volumes, but a brief list of the subjects treated will indicate their contents.

Following an account of the expedition and an historical survey of the islands we have articles on magnetics; on the radium content of certain igneous rocks; on the meteorology and geology of Campbell Island; on the physiography, geology, soil and soil formers of the various islands; on the vertebrates, mollusca and general entomology; special articles on macrolepidoptera, lepidoptera, hymenoptera, coleoptera, diptera, collembola, spiders, crustacea, polychæta, oligochæta, echinoderms, holothurians, planarians, nemerteans, leeches, myriapods, medusæ, actinians, sponges and foraminifera. In botany articles are provided on systematic and ecologic botany, plant formations and associations, grasses, algæ and cryptogams. A summary of the biological relations of the islands, by the editor, a bibli-

[1] The volumes may be had of Dulau & Co., 87 Soho Square, London, the agents of the Philosophical Institute of Canterbury, New Zealand.

ography, an excellent index and a general map of the Antarctic and sub-Antarctic regions concludes the work.

Much of the land mass of the islands is of igneous or granitic rocks, but fossils of tertiary age, in limestone, have been found on Campbell Island and all the conditions indicate the probability that all the islands formed part of a continental area connecting them with New Zealand. Wingless species or species with reduced wings are numerous among the insects, as might be expected.

In his general review the editor leans toward the theory of a great Antarctic continent in the warmer Tertiary time—with connections or close relations with Patagonia, South Africa and Australasia—as best explaining the distribution of animal and plant life now existing and the fossil remains which have been collected in the Antarctic and sub-Antarctic regions.

Wm. H. Dall

ANNUAL INTERNATIONAL TABLES OF PHYSICAL AND CHEMICAL CONSTANTS

At the International Congress of Applied Chemistry, held in London in 1909, an international commission was appointed with power to undertake the publication of "Annual Tables" containing all constants and other numerical data which may be of interest in physics, chemistry or in the technical applications of these sciences. The plans outlined by the commission received the endorsement of the International Association of Academies and the official recognition and financial support of many of the governments and learned academies of the world. Since its inception the commission has been enlarged and made more thoroughly representative of the various branches of science. It is now composed of twenty-five chemists and physicists representing the following countries: Austria, Belgium, Great Britain, France, Germany, Holland, Italy, Russia, Scandinavia, Spain, Switzerland and the United States.

Owing to the immense volume of scientific and technical literature which is continually being produced, the difficulties in the way of ascertaining whether any given measurement has been made or not are increasing year by year. Existing systems of indexing and abstracting offer only limited help, since a large number of measurements are made in the course of researches to which they are purely subsidiary, so that their existence can not be inferred from the titles and subtitles of the papers in which they are recorded. Also, tables which appear only at long intervals, such as those of Landolt and Börnstein, can of necessity cover only a small part of the ground and are never quite up to date. The "Annual Tables" should therefore fill a serious gap which has hitherto existed in the systematic indexing of scientific and technical results.

During the year 1910 all scientific publications which might contain material of value were systematically scrutinized by a large corps of abstractors. From the data thus obtained a volume of tables and bibliography is about to be published, covering the year 1910. The volume will form a valuable addition to the physical chemical tables which already exist and will, together with the succeeding annual volumes, prove a powerful aid to the work of the investigator, both in pure and applied science, and will enable him to find with ease those data which he may require and which it would be most difficult to obtain by individual search. Many important constants published in little used journals, or in papers which are inadequately indexed, will be saved from oblivion. Each value, given in the tables, will be accompanied by the name of the author, by a reference to the original paper and by an indication of the exact conditions under which the measurements were made. The text of the tables will be printed in English, German, French and Italian.

The committee urgently requests authors of scientific papers to cooperate with them by sending to one of their number (two) reprints of all articles published. This is especially desirable in the case of papers published in the form of theses, of government

reports, or in any journal which might not come to the attention of the abstractors.

It is expected that the publication of the tables will, after three or four years, become self-supporting. In the meantime, generous subscriptions have been made by governments, academies, scientific societies and individuals throughout the world; but further subscriptions must be obtained before the continued success of the project is assured. The general secretary, Dr. Charles Marie, 98 Rue du Cherche-Midi, Paris, and the members of the International Commission serve without remuneration.

Information concerning the "Annual Tables" or the work of the international committee will be gladly furnished by the undersigned American members of the committee.

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SPECIAL ARTICLES

CHEMISTRY OF THE SILVER VOLTAMETER[1]

Among the questions relating to the chemistry of the silver voltameter which have been investigated more or less in detail are the following: (1) Effect upon electrolyte of the various septa employed in the different types of voltameters to separate the anode electrolyte from that of the cathode; these septa include (*a*) filter paper, (*b*) silk, and (*c*) porous pots of unglazed porcelain; (2) the effect of various kinds of impurities upon the weight of the silver deposit and the explanation of this effect; (3) the preparation and testing of pure silver nitrate free from traces of impurities which produce disturbing effects in the voltameter; (4) anode secondary reactions; (5) cathode secondary reactions; (6) preparation of the silver anode; (7) purity of the silver deposit.

[1] Read before the Philosophical Society of Washington, May 20, 1911.

Of these, the first question has been studied in greatest detail, principally because of the fact that it includes the cardinal differences between the various types in use by national standardizing laboratories. It early became evident that the different results obtained with the various types was due principally to the effect produced by these septa, and that two of them introduced errors of much greater magnitude than any ordinary variations in the conditions or in the purity of different samples of even commercially pure silver nitrate. However desirable it might have been to have devoted every energy to the preparation of pure electrolyte and to its protection from contamination during the experiments, it was nevertheless necessary first to show, *if possible,* just what the nature of the action of the septa might be, since the primary object of the work was a *study of the silver voltameter as actually used,* and especially as used by the various standardizing laboratories, with a view of determining a uniform type if possible. The results of the investigation of the effect of filter paper seem to show that ordinary filter paper is superficially covered with oxycellulose, which can be extracted with water but which again forms spontaneously when the filter paper is allowed to remain in contact with the air. This oxidation is probably due to fermentation. This oxycellulose solution (colloidal) very readily reduces silver nitrate solution to colloidal metallic silver, which is very similar in properties to the colloidal silver of Carey Lea. Permanent colloidal solutions of silver have been prepared from concentrated aqueous extracts of filter paper. This reduction of silver nitrate is probably due to the intermediate formation of *furfuraldehyde* since the oxycellulose solution is readily decomposed into this aldehyde by the action of exceedingly dilute nitric acid of no greater concentration than that which is probably present in neutral silver nitrate solution (due to slight hydrolysis). Furfuraldehyde, especially the polymerized variety, produces all the *peculiar* effects which have been observed with filter paper, *e. g.,* imparts to the electro-

deposited silver a strongly striated and noncrystalline appearance. Other *strong* reducing agents produce similar effects, but to a less extent. Cane sugar and starch do not produce these effects.

It is very probable that the final effect of the filter paper in increasing the weight of the silver deposit is principally due to the deposition of the *colloidal* silver (by cataphoresis) and also some of the protective or "schutz" colloid-oxycellulose upon the cathode along with the electrolytically deposited silver, resulting in a breaking up of the usual crystalline form of the silver so that it occludes greater amounts of electrolyte. Thus, an unweighable amount of colloid can increase the weight of the silver deposit by a quite appreciable amount. Of course, the weight is further increased by the actual mass of the colloid deposited, but this seems to be a small per cent. of the total increase.

Silk when first used produces an effect very similar to that of filter paper, due to the ease with which it is partially decomposed into aldehydes. After repeated use in the voltameter, this aldehyde decomposition ceases, and it renders the electrolyte strongly acid, probably due to its decomposition into amino-acids. Since, in general, the effect of acid is to decrease the weight of the silver deposit, this fact probably accounts for the progressively decreasing values obtained with a voltameter with silk septa as compared to those obtained with the porous pot type.

Porous pots, when prepared according to certain specifications, have practically no effect upon the electrolyte so far as could be determined. If not so prepared, they render the electrolyte slightly acid and very faintly reducing in character. This *very slight* action is probably catalytic in character, and probably consists in a slight reduction of the silver nitrate to colloidal silver with the formation of an equivalent amount of nitric acid. This action lies at the basis of electrostenolysis. Of all the septa, the porous pot is by far the most inert.

In addition to colloidal silver, certain other colloids produce similar effects, as colloidal silica and colloidal silver hydroxide, whereas other colloids like hydrocarbons and starch do not. It is evidently a matter of whether the colloid migrates to anode or cathode *under the conditions* which *obtain in the voltameter.* This direction of migration may of course be different under other conditions.

Next to the presence or absence of strongly reducing impurities or certain colloids, the condition of *neutrality* of the electrolyte has the greatest effect upon the results. A method has been developed for defining the neutrality to within a part in a million of nitric acid. Fused silver nitrate when prepared according to the methods described in the literature is both slightly basic and also contains minute traces of reducing impurities which are very objectionable in voltameter work. Recrystallized silver nitrate (from neutral solution) is slightly basic, consequently the recrystallization must be made from acid solutions. This acid may be removed by recrystallization or by careful fusion, but the neutrality must be tested in either case.

There is no evidence of any secondary reactions at the cathode. The anode solution remains practically neutral, but if appreciably acid at the start tends to become neutral. The same is true when no septum is used. This indicates a secondary reaction of some sort at the anode. No evidence was found of the formation of reducing substances at the anode, such as a sub-silver nitrate. The evidence obtained by other investigators of the formation of such substances seems to be at least partially vitiated by the fact that in some cases at least the anode solution was filtered through filter paper. In other cases, the character of the filter is not mentioned.

No considerable work has as yet been done on the last question (7).

In the future work, the electrolyte will be subjected to ultramicroscopic examination for the presence of colloids.

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